

Interactive comment on “The SPARC water vapour assessment II: Profile-to-profile and climatological comparisons of stratospheric $\delta D(H_2O)$ observations from satellite” by Charlotta Högberg et al.

Anonymous Referee #2

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Review of "The SPARC water vapour assessment II: Profile-to-profile and climatological comparisons of stratospheric $\delta D(H_2O)$ observations from satellite" by Charlotta Högberg et al.

Högberg et al. compare $\delta D(H_2O)$ observations from three different satellites in the stratosphere. The paper is well written and it provides a detailed analysis of H_2O , HDO and $\delta D(H_2O)$ measurements from the three instruments. Such work is important for the scientific use of these kind of data sets and I really think the paper should be published, however, my reason for clicking on "reject" mainly bases on the authors' journal choice.

C1

I.e. to my understanding, this is not an "ACP paper", but see my main point below for more details on this. A few minor points that should be considered before publication are also listed below.

Major point

- In my opinion, an ACP paper is supposed to tackle a question/issue/problem of a physical or a chemical process in the atmosphere. That is not done at all in this study. It is (merely) the comparison of three different data products. A lot can be learned from this about retrieval parameters, about effects of spatial and temporal sampling or of using different frequency bands, and so on, but here nothing is learned about the physics or the chemistry of the atmosphere. Hence, I suggest to withdraw the paper from ACP and to submit it e.g. to AMT or a related journal. Those journals are also well-renowned and provide room for rather technical papers like this one here.

Minor points

- P3L31andL32: "boreal winter and "boreal" summer
- P4L7-8: That is very simplified, the issue is a lot more nuanced. Please see Frank et al. 2018, 10.5194/acp-18-9955-2018
- P5L10: There should be more recent literature for this than from 1996
- P5L28 and P6L5: Eichinger et al. 2015 (10.5194/acp-15-5537-2015) dealt with this issue in a model-satellite comparison
- P6L16: Explain what LT means.
- Sect. 4: Why do you start with showing biases and not the actual profiles first? I find that confusing.

C2

- I agree with what Mr. Johnson says that it is hard to say what can actually be learned from this study, since there are so many differences between the different methods, one cannot actually see any causalities. I would also appreciate some sort of conclusion that at least states this product/instrument is better here, and this is worse there. Which product can be trusted more where and/or when for making process studies or model comparisons? And maybe also methodologically, which method is best for what? In a future satellite mission, how would the "best" instrument look like, and how can the retrievals be improved?
- For several reasons the paper is pretty lengthy and that could easily be reduced: The information in Sects. 2 and especially 3 should be reduced to the most important points, technical details and the bulk of the equations should be banished to the supplement. Moreover, the paper is pretty repetitive, e.g. the (first part of the) discussion and the conclusion are not more than summaries of the results. Some restructuring and removing can easily resolve this.

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